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Center # : 10/24-6-R7312-0A0 Center shr #: 10/22-1-F7312-0A0 OCA file #:
Work type : RES
Contract#: DDM-9114554 Mod #: INITIATION Document : GRANT
Prime #: Contract entity: GTRC

Subprojects ? : N CFDA: 47.041
Main project #: PE #:

Project unit: ISYE Unit code: 02.010.124
Project director(s):
TSUI K-L ISYE (404)894-2311

Sponsor/division names: NATL SCIENCE FOUNDATION / GENERAL
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Award period: 910901 to 940228 (performance) 940531 (reports)

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Contract value	75,000.00	75,000.00
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Does subcontracting plan apply ?: N

Title: DEVELOPMENT OF NEW EXPERIMENTAL DESIGN METHODS FOR QUALITY IMPROVEMENT

PROJECT ADMINISTRATION DATA

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Security class (U,C,S,TS) : U
Defense priority rating :
Equipment title vests with: Sponsor GIT X

ONR resident rep. is ACO (Y/N): N
supplemental sheet

Administrative comments -
PROJECT INITIATION

GEORGIA INSTITUTE OF TECHNOLOGY
OFFICE OF CONTRACT ADMINISTRATION

NOTICE OF PROJECT CLOSEOUT

Closeout Notice Date 06/06/94

Project No. E-24-641 Center No. 10/24-6-R7312-0A0

Project Director TSUI K-L School/Lab ISYE

Sponsor NATL SCIENCE FOUNDATION/GENERAL

Contract/Grant No. DDM-9114554 Contract Entity GTRC

Prime Contract No. ______

Title DEVELOPMENT OF NEW EXPERIMENTAL DESIGN METHODS FOR QUALITY IMPROVEMENT

Effective Completion Date 940228 (Performance) 940531 (Reports)

Closeout Actions Required:	Y/N	Date Submitted
Final Invoice or Copy of Final Invoice	N	
Final Report of Inventions and/or Subcontracts	N	
Government Property Inventory & Related Certificate	N	
Classified Material Certificate	N	
Release and Assignment	N	
Other ______	N	

CommentsLETTER OF CREDIT APPLIES. 98A SATISFIES PATENT REQUIREMENT.

Subproject Under Main Project No. ______

Continues Project No. ______

Distribution Required:

Project Director	Y
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PI/PD Name and Address

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NATIONAL SCIENCE FOUNDATION
FINAL PROJECT REPORT

PART I - PROJECT IDENTIFICATION INFORMATION

1. Program Official/Org. Pius J. Egbelu - DMII

2. Program Name OPERATIONS RESEARCH & PRODUCTION SYSTEMS

3. Award Dates (MM/YY) From: 09/91 To: 02/94

4. Institution and Address

GA Tech Res Corp - GIT
Administration Building
Atlanta GA 30332

5. Award Number 9114354

6. Project Title

Development of New Experimental Design Methods for Quality Improvement

This Packet Contains
NSF Form 98A
And 1 Return Envelope

PART IV -- FINAL PROJECT REPORT -- SUMMARY DATA ON PROJECT PERSONNEL

(To be submitted to cognizant Program Officer upon completion of project)

The data requested below are important for the development of a statistical profile on the personnel supported by Federal grants. The information on this part is solicited in resonse to Public Law 99-383 and 42 USC 1885C. All information provided will be treated as confidential and will be safeguarded in accordance with the provisions of the Privacy Act of 1974. You should submit a single copy of this part with each final project report. However, submission of the requested information is not mandatory and is not a precondition of future award(s). Check the "Decline to Provide Information" box below if you do not wish to provide the nformation.

Please enter the numbers of individuals supported under this grant.
Do not enter information for individuals working less than 40 hours in any calendar year.

	Senior Staff		Post-Doctorals		Graduate Students		Under-Graduates		Other Participants[1]	
	Male	Fem.	Male	Fem.	Male	Fem.	Male	Fem.	Male	Fem.
A. Total, U.S. Citizens	1									
B. Total, Permanent Residents										
U.S. Citizens or Permanent Residents[2]: American Indian or Alaskan Native										
Asian.					1					
Black, Not of Hispanic Origin.										
Hispanic										
Pacific Islander										
White, Not of Hispanic Origin										
C. Total, Other Non-U.S. Citizens										
Specify Country 1.										
2.										
3.										
D. Total, All participants (A + B + C)	1				1					
Disabled[3]										

☐ Decline to Provide Information: Check box if you do not wish to provide this information (you are still required to return this page along with Parts I-III).

[1] Category includes, for example, college and precollege teachers, conference and workshop participants.

[2] Use the category that best describes the ethnic/racial status fo all U.S. Citizens and Non-citizens with Permanent Residency. *(If more than one category applies, use the one category that most closely reflects the person's recognition in the community.)*

[3] A person having a physical or mental impairment that substantially limits one or more major life activities; who has a record of such impairment; or who is regarded as having such impairment. *(Disabled individuals also should be counted under the appropriate ethnic/racial group unless they are classified as "Other Non-U.S. Citizens.")*

AMERICAN INDIAN OR ALASKAN NATIVE: A person having origins in any of the original peoples of North America and who maintains cultural identification through tribal affiliation or community recognition.

ASIAN: A person having origins in any of the original peoples of East Asia, Southeast Asia or the Indian subcontinent. This area includes, for example, China, India, Indonesia, Japan, Korea and Vietnam.

BLACK, NOT OF HISPANIC ORIGIN: A person having origins in any of the black racial groups of Africa.

HISPANIC: A person of Mexican, Puerto Rican, Cuban, Central or South American or other Spanish culture or origin, regardless of race.

PACIFIC ISLANDER: A person having origins in any of the original peoples of Hawaii; the U.S. Pacific territories of Guam, American Samoa, and the Northern Marinas; the U.S. Trust Territory of Palau; the islands of Micronesia and Melanesia; or the Philippines.

WHITE, NOT OF HISPANIC ORIGIN: A person having origins in any of the original peoples of Europe, North Africa, or the Middle East.

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May 23, 1994

Mrs. Carol Guido, Administrative Officer
National Science Foundation
4201 Wilson Boulevard
Arlington, VA 22230

Dear Mrs. Guido:

Enclosed is the final report of my NSF project DDM-9114554, "Development of New Experimental Design Methods for Quality Improvement". This report contains a complete description of the research I have conducted under this NSF project. As shown in the report, we have enjoyed tremendous success in our research effort on developing improved statistical methods for designing and analyzing robust design experiments.

Please let me know if you need anything else from me. Thank you very much.

Sincerely,

Kwok-Leung Tsui

FINAL REPORT — Grant DDM-9114554
Development of New Experimental Design Methods for Quality Improvement

Kwok-Leung Tsui

May 25, 1994

Abstract

This paper reports on the research we have conducted over the last two years under NSF Grant DDM-9114554. The general goal of our research is to develop effective, statistically efficient, and user-friendly techniques and tools for robust design problems. We have developed improved statistical methods for designing and analyzing robust design experiments. For planning experiments, we developed a new experiment format, the *combined array format*, which can reduce the experiment size and allow greater flexibility for estimating effects which may be more important for physical reasons. We also developed alternative design strategies, graphical tools and tables, and computer algorithms to help engineers plan more efficient experiments. For analyzing experiments, we developed a new modeling approach, the *response model approach*, which yields additional information about how control factor settings dampen the effects of individual noise factors; this helps engineers better understand the physical mechanism of the product or process. We also developed alternative variability measures for Taguchi's signal-to-noise ratios and methods for empirically determining the appropriate measure to use.

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1 Introduction

Robust Design is an important method for improving product quality, manufacturability, and reliability at low cost. The main idea of robust design is to reduce the output variation from target (the desired output) by making the performance insensitive to noise factors such as manufacturing imperfections, environmental variations, and deterioration. Taguchi's introduction of this method (Taguchi, 1986) in 1980 to several major

American industries resulted in examples of significant quality improvement in product and manufacturing process design.

In planning robust design experiments, Taguchi proposed construction of two separate experiment plans for the control and noise factors as the experimental format. As shown in Shoemaker, Tsui, and Wu (1991) and other papers, this format leads to less flexible and unnecessarily expensive experiments. Shoemaker et al. (1991) called this experimental format the product array format and showed that this experimental format dictates estimation of many effects that are unlikely to be important and thus increase experiment cost. An alternative experimental format is to combine the control and noise factors and plan a single experiment; this is called the combined array format. Welch et al. (1990) first illustrated the run savings of this new experimental format with a computer experiment. Additional discussions and comparisons of the two experimental formats can be found in Box and Jones (1990,1992) and Lucus (1989).

For experimental design criteria and tools, Taguchi recommended that the experimenter only use orthogonal arrays, linear graphs, and interaction tables for planning experiments. As pointed out in Kacker and Tsui (1990) and Tsui (1992), there exist alternatives tools that are more effective and user-friendly.

In analyzing robust design experiments, Taguchi proposed to first calculate the signal-to-noise (SN) ratio for each combination of the control factors, and then identify the "optimal" factor settings which maximize the SN ratios. Shoemaker, Tsui, and Wu (1991) called this analysis approach the loss model approach since it is equivalent to fitting a model to a chosen loss measure or SN ratio. The loss model approach is appealing because it provides direct estimates of the loss measure based on the data. However, an appropriate statistical model for the loss measure may often be complicated since loss is a nonlinear and many-to-one transformation of the observed response. Recently, much work has been done to discuss the problems of this modeling approach and to consider alternative modeling approaches.

A natural alternative modeling approach is to first model the observed response, and then use this fitted response model to minimize loss. Shoemaker et al. (1991) called this modeling approach the response model approach. Welch et al. (1990) first proposed a formal strategy for this approach and illustrated the approach with a computer experiment. Shoemaker et al. extended this approach and illustrated its advantages through a physical experiment. Box and Jones (1990) considered a more general class of loss measures and proposed economical experiment designs specially suited to minimization of these measures. Lucas (1989) and Myers, Khuri and Vining (1992) applied response surface methodology to the robust design problem, and Lucas developed "mixed resolution" composite designs for these applications. Freeny and Nair (1991) developed alternative analysis methods for situations where the noise variables are uncontrolled but observable. Montgomery (1991) applied the response model approach and simple residual analysis techniques to improve the robustness of an industrial process. Shoemaker and Tsui (1993) developed a formal basis for the graphical data-analytic approach presented in Shoemaker et al. They decomposed overall response variation into components representing the variability contributed by each noise factor, and showed when the decomposition allows the experimenter to use individual control-by-noise interaction plots to minimize response variation.

As pointed out in Box and Jones (1990), Myers, Khuri, and Vining (1992), and Shoemaker and Tsui (1993), the variance of response due to noise can be quadratic in the control factors even if the response is linear in the control factors. Tsui (1994a) studied the consequences of this fact and discussed the major problem of the loss model approach. He showed that the use of the loss model approach in highly fractionated experiments may create unnecessary biases for the factorial effect estimates, and thus may lead to non-optimal solutions. Steinberg and Bursztyn (1994) addressed the same problem and presented with real examples.

Besides the bias problem, Shoemaker et al. (1991) pointed out that the loss model approach suffers the problem of information loss since it does not provide information on the effects of individual noise factors. In contrast, the response model approach directly models the response over the control and noise factors and thus can explain how the control factors dampen the effects of the individual noise factors.

In addition, the loss model approach often has a lower statistical power in detecting the dispersion effects compared to the response model approach. Steinberg and Bursztyn (1993) showed this through analytical arguments and numerical examples. Li and Tsui (1994) studied the efficiency loss of the loss model approach under various underlying true models for the response.

This paper reports on the research we have conduced under NSF Grant DDM-9114554. We have enjoyed tremendous success in our research efforts on developing improved statistical methods for designing and analyzing robust design experiments. Sections 2 to

4 discuss alternative methods for designing robust design experiments which include alternative experimental format, experimental design criteria and strategies, and design techniques and tools. Section 5 and 6 discuss alternative methods for analyzing robust design experiments, which include optimization strategies and modeling approach. Section 7 discusses the real-world applications of the proposed methods. Section 8 summarizes the paper.

2 Alternative Experimental Format

Taguchi proposed construction of two separate experiment plans for the control and noise factors. We say that the experiment plan for the control factors is the control array, that for the noise factors is the noise array, and the experiment set-up is the product array format. In general, a product array experiment contains two experiment plans, one for control factors and one for noise factors. In the experiment, the control factors are varied according to the combinations of the control array; and for each row of the control array, the noise factors are varied according to the combinations of the noise array.

The product array experiment format is conceptually simple and appealing to engineers. It also provides a direct estimate of quality loss from the data, which is valuable when the statistical modeling method fails to result in quality improvement. However, this experiment format can require a large number of runs because the noise array is repeated for every row in the control array. Also, it uses a large number of degrees of freedom to estimate all possible interactions between control factor effects and noise factor effects. This limits the flexibility of using some degrees of freedom to estimate other more important effects.

To rectify these disadvantages, a natural alternative format is to combine the control and noise factors together and plan a single experiment; this is called the combined array format. Welch et al. (1990), Shoemaker, Tsui, and Wu (1991), Box and Jones (1990), and Lucas (1990) discussed this new approach and explained with many examples how this approach can lead to run savings and more flexibility.

Below we modify the example in Engel (1992) to illustrate the possible run savings of the combined array approach over the product array approach. Suppose in planning the experiment, the engineers have identified five control factors, A, C, D, E, and G and two noise factors, N and O. In order to save experimental runs, they need to make some assumptions about interactions. Suppose engineering knowledge leads them to believe that there is no potentially significant control by control interaction. To run a product array experiment, they need to construct a control array for the five control factors and a noise array for the two noise factors. The smallest fractional factorial design for five factors is a eight-run design and the design for two factors is a four-run design and the product array will be a 32-run design. The design in the original experiment in Engel (1992) is a candidate for such a product array design. This array will allow the estimation of the main effects of the five control factors and two noise factors, the estimation of all control-by-noise interactions, the estimation of control-by-control interaction AG and some third or higher order interactions.

To run a combined array, we assume that third and higher order interactions are negligible which is quite common in industrial experimentation. It follows that the only effects we need to estimate are {A,C,D,E,G,N,O,AG,AN,CN,DN,EN,GN,AO,CO,DO,EO, and GO} which account for 18 degrees of freedom. It is easy to follow the algorithm in Mitchell (1974) to generate a 20 or 22 run D-optimal design to estimate these effects. (See Shoemaker et al. (1991) for an example.) These D-optimal designs have been shown to have similar efficiency as the orthogonal array designs.

Suppose additional knowledge leads the engineers to believe that the control factors can be divided into two groups: $\{A, D, G\}$ and $\{C, E\}$, noise factor O interacts only with the first group, and N interacts only with the second group, i.e., only $\{AO, DO, GO, CN$, and $EN\}$ are potentially significant. Then a 16-run orthogonal design can be constructed to estimate all main effects and these potentially significant interactions. A 2^{7-3}_{III} with generators $I = ADE = CDG = AGNO$ is a candidate for such a design; and this design happens to be a half fraction of the product array design in Engel (1992). As shown in Tsui (1994b), this design provides the same information as the product array design but requires only half of the experiment runs. In general, if additional engineering knowledge is available, a combined array can save experiment runs over the product array and still provide similar information. Box and Jones (1990), Lucas (1989), Shoemaker, Tsui, and Wu (1991), and Welch et al. (1990) provide additional examples on run savings and flexibility of the combined array approach.

Note that the 16-run experiment used above is not the best experimental design based on the maximum resolution criterion. The experiment was chosen so that it is a half fraction of the product array experiment; and thus the old experiment can be re-analyzed.

A 16-run resolution IV experiment with generators $I = ACDE = ACGN = CDGO$ will allow the estimation of the same effects described earlier with better resolution. Many techniques can be used to construct such candidate designs that have a smaller run size but allow the estimation of the specified interactions. (See Tsui (1988), Kacker and Tsui (1990), Wu and Chen (1992) for detail.)

Although the combined array format has several advantages, it generally requires more work than the product array format. When using the combined array format, the experimenter needs to decide what control by noise interactions are required to estimate in the experiment. Nevertheless, this does not limit the use of the combined array format. Instead, this leads to the development of new designs and modifications of existing designs. Many of the common designs, such as response surface designs (Lucas, 1990), split-plot designs (Box and Jones, 1992), and nested designs, are potentially useful for the combined array experiments. More research should be done to study and tailor these designs and to develop analysis strategies of these designs for robust design problems. This would ease the application of combined array experiments and result in experiment run savings and cost reduction.

3 Alternative Design Criteria and Strategies

For the control array, Taguchi assumed that there are no interactions among control factors and proposed to choose specific responses and control factor settings to meet this assumption (see Phadke and Taguchi, 1987). Because of this assumption, the experimenter can study a large number of control factors in a small experiment and thus save experment cost. For the noise array, Taguchi recommended that the experimenter study the noise effect by testing each noise factor at two or three settings.

For experimental design tools, Taguchi recommended that the experimenter only use orthogonal arrays, linear graphs, and interaction tables for planning experiments. An experiment plan is constructed by: (i) choosing an orthogonal array, (ii) customizing the array by various techniques including combining columns and collapsing settings, (iii) assigning factors to columns, and (iv) deleting unassigned columns. Linear graphs and interaction tables were developed to help customize orthogonal arrays. If the chosen orthogonal array does not lead to the desired result, the experimenter may iterate the process by selecting another array until an appropriate plan is obtained.

It is a good idea to identify fundamental responses or control factors in order to simplify the functional relationship in the response surface. In addition, transformation of responses and/or factor values, such as taking logarithm of the data (see Box and Cox, 1964; Box and Tidell, 1962), is another technique to simplify the response surface function. However, sometimes neither of these techniques can reduce the response surface function to an additive model so that we need to allow estimation of some interactions in planning experiments. In these situations, design criteria become important when choosing appropriate experimental designs.

In planning fractional factorial experiments, maximizing resolution (see definition in Box and Hunter, 1961) and minimizing aberration (see definition in Fries and Hunter, 1980) are the most common design criteria. These criteria basically assume that interactions of the same order are equally important and lower order interactions are more important than higher order interactions. Greenfield (1976) and Franklin and Bailey (1977) proposed a different criterion which seeks a plan that allows the main effects and a specified set of interactions, called a "requirement set", to be estimable without being confounded with each other. (The "requirement set" usually includes all main effects and all potential interactions that the experimenter wants to study.) This criterion assumes that the experimenter has some prior knowledge about the interactions among factors.

These two sets of criteria carry different assumptions and may conflict with each other. One may prefer the maximum resolution criterion by arguing that the experimenter never has prior knowledge about the interactions before the experiment is run. On the other hand, some engineers prefer the "requirement set" criterion since they believe their prior knowledge about which interactions are likely to be important would reduce the experiment size. Tsui (1992) illustrated with examples how prior knowledge on interactions can be available and how the requirement set criterion may conflict with the maximum resolution criterion. Amster and Tsui (1993) discussed a different criterion where the main effects and 2-factor interactions can always be de-aliased.

Other than design criteria, some important ideas in classical experimental design, such as blocking and replication, are relevant in robust design problems.

Similar to running other types of experiments, replication is very useful for detecting the size of experimental error when running robust design experiments. However, the idea of using replication to capture the effect of noise is inefficient and generally not recommended.

Noise is classified as the variable which is impossible, undesirable, or uneconomical to control (see Abraham and Mackay, 1990 for good examples on noise). The objective of robust design is to reduce variation by dampening the effect of noise. In traditional experiments, the effect of noise is often measured and studied by taking replications. This technique, however, is often not appropriate in robust design problems. Instead, the experimenter should always spend effort to understand and identify all possible sources of noise (e.g., using a process capability study) before conducting the robust design experiment. These major noise variables should then be systematically varied in the experiment. This approach is more efficient than taking replications, allows us to better understand the sources of process variation, and eases the task of model diagnostics when the robust design experiment fails. Shoemaker and Tsui (1992a), Steinberg and Bursztyn (1993), and Tsui (1992, 1994a, 1994b) provide further discussions and illustrations on this idea.

Sometimes noise variables are uncontrollable during the experiment even though they can be identified. In this case surrogate noise factors can be used to systematically study the effect of these identified but uncontrollable noise variables. For example, temperature and gas composition variations inside a reactor can be studied by using position in the reactor as a surrogate noise factor (see Kackar and Shoemaker, 1986).

The active introduction of noise increases the efficiency of the experiment as it is very similar to the idea of blocking in classical designs of experiments. Blocking is a frequently used experimental design technique for increasing statistical power of detecting significant factorial effects. Blocking factors are usually identified as the variables which significantly contribute to variation. Systematically varying the blocking factors, rather than allowing them to vary randomly, will significantly reduce the experimental error and thus increase the power of detecting factorial effects. However, as pointed out in Shoemaker and Kacker (1988) and Tsui (1992), there is a fundamental difference between the blocking factors and noise factors. Implicitly, blocking factors are assumed not to interact with control factors (design parameters) so that the blocking effect will not interfere with the factorial effects of interest. In contrast, the noise factors are anticipated to interact with the control factors so that the experimenter can choose the appropriate control factor settings to dampen the effect of noise.

4 Alternative Design Techniques and Tools

Taguchi recommended the use of orthogonal arrays, linear graphs, and interaction tables for planning experiments. These tools have been very successful since engineers can learn how to use these tools without a strong background in statistics. This way, engineers can plan experiments themselves without depending on statisticians. As the dependence on a limited number of statisticians has been the bottleneck for popularizing the use of experimental design in industry, development of simple experimental design tools could resolve this bottleneck and increase the use of experimental design dramatically.

Clearly, orthogonal arrays, linear graphs, and interaction tables are not the only simple tools that could ease the engineer's job for planning experiments. Below we discuss three categories of alternative tools which either are easier to use and give better statistical properties or provide more guidance to engineers for planning experiments.

The orthogonal arrays popularized by Taguchi are only a small subset of orthogonal arrays that are useful in industrial applications. They are popular because they are reasonably small and flexible for customization. Among these arrays, regardless of their complex smearing alias structures, OA12 and OA18 (called L12 and L18 in Taguchi, 1986) have been extremely popular because of their flexibility for studying many (mixed setting) factors. Sherry (1988) and Hamada and Wu (1991a) studied the complex alias structures of these arrays, and the latter developed strategies for analyzing the corresponding experiment data.

Using orthogonal arrays is only one way of constructing orthogonal designs. The well-known approach discussed in Box, Hunter, and Hunter (1978) is a good alternative for constructing fractional factorial designs and understanding confounding relationships systematically. In addition, other simple techniques, such as those discussed in Dey (1985) and Wang and Wu(1989, 1991), are very useful alternatives and provide more flexibility for planning small orthogonal experiments.

Taguchi (1986) proposed linear graphs as tools for planning orthogonal array experiments. As pointed out in Wu and Chen (1992), these graphs are incomplete for larger orthogonal arrays and with non-optimum resolution. Kacker and Tsui (1990) developed interaction graphs which summarize all interaction relationships of orthogonal arrays. Unlike linear graphs that consist of many graphs for each orthogonal array, there is only a single interaction graph for each orthogonal array. Using interaction graphs, engineers can easily plan orthogonal array experiments and identify confounding relationships of the planned experiment. Interaction graphs are particularly useful when the experimenter needs to make trade-offs among factorial effects. Wu and Chen (1992) proposed optimal line graphs which offer substantial improvement over Taguchi's linear graphs. Optimal line graphs consist of a complete list of all possible interaction patterns of the corresponding orthogonal array. Maximum resolution and minimum aberration are built-in in these graphs, which allow the experimenter to obtain plans with optimal statistical properties. Tsui (1988) developed confounding tables for planning experiments and identifying confounding relationships. Unlike interaction graphs and optimal line graphs, these tables are available for three-level orthogonal arrays. They are particular useful for implementation in computer systems because of their tabular form.

All of these new tools improve Taguchi's linear graphs and help engineers plan better experiments by themselves. However, unlike linear graphs and confounding tables, interaction graphs and optimal line graphs are currently limited to two-level designs. It will be useful to generalize these graphical tools for three-level or more general designs.

As mentioned earlier, easing the job of planning experiments is the key factor for popularizing the use of experimental design in industry. In addition to developing more convenient and efficient tools, another alternative is to develop computer software to further ease the job of planning experiments. Franklin (1985) developed an algorithm for constructing equal-setting fractional factorial designs that satisfy the "requirement set" criterion. By modifying Franklin's idea and using orthogonal array customization techniques, Tsui (1989) developed an algorithm for constructing orthogonal array experiments for mixed setting factors. This algorithm had been implemented in the automatic experiment planner (AEP) module of the AT&T Quality Workbench — ROBUST, a software product to help engineers run robust design experiments. The AEP module requests from users minimum input information (including the "requirement set" and the experiment budget) and gives as output orthogonal array plans with good statistical properties. This module has been well-received by many AT&T engineers as a very efficient tool for planning experiments. Although other designs can be added in the future, the current system is limited to constructing common orthogonal array experiments.

Haaland et al. (1988) and Lorenzen and Truss (1989) developed computer software packages with built-in strategies and rules for planning experiments. These packages require more information from users but can construct more flexible experiments. Developing computer software for planning experiments certainly helps popularize the use of experimental design among engineers. It will be important to develop tools that simultaneously incorporate engineering knowledge, statistical experimental design expertise, and ease of computer implementation. Koch et al.(1991) summarizes sources and information for most available commercial robust design software including ROBUST.

5 Alternative Optimization Strategies for Robust Design

Taguchi classified robust design problems according to their response objective to smaller-the-better (STB), nominal-the-best (NTB), and larger-the-better (LTB) problems (see Taguchi (1986) for definitions). For the NTB problems, Taguchi proposed a two-step procedure for identifying the "optimal" factor settings: first find factor settings which maximize the signal-to-noise (SN) ratio, then bring mean response to target by changing the adjustment factor (a factor that has a large effect on the mean but no effect on the SN ratio). The SN ratio here is defined to be 10 log $(\bar{Y}^2/S^2)$, with $\bar{Y}$ and S^2 being the sample mean and variance of the observed responses. For the STB and LTB problems, Taguchi proposed to identify the "optimal" factor settings by simply maximizing their corresponding SN ratios, which are defined to be -10 log $(n^{-1}\sum Y_i^2)$ and -10 log $(n^{-1}\sum 1/Y_i^2)$, respectively, where $Y_1, ..., Y_n$ are the observed responses.

Within NTB we can define two further categories; cases in which there is an adjustment factor than can move the mean to target without much effect on variability, and cases in which the mean cannot be moved to target without a large effect on variability.

For the NTB case with adjustment factors, the two-step procedure proposed by Taguchi reduces the dimension of the original optimization problem and allows future changes of target value without re-optimization (see Leon, Shoemaker, and Kacker, 1987 for more explanation on these advantages). While this two-step procedure has been recognized to be very useful in practice, there is no reason to always optimize the SN ratio in the first step. The SN ratio is a monotone function of the coefficient of variation, which is a particular measure of variability. In general, different measures of variability should be optimized in the first step for different underlying true models. Leon et al. (1987) show how different underlying response models lead to different variability measures, and Box (1988), Nair and Pregibon (1987), and Tsui (1990) propose data analytic methods for estimating variability measures from experimental data. Tsui (1990) generalizes the SN Ratio by considering $\log(variance)/| mean |^{\alpha}$, and Box (1988) considers $\log(variance(y^{\lambda}))$, where y^{λ} represents the Box-Cox family of transformations. A key criterion for choosing α or λ is the ability to identify an adjustment factor. All these alternatives make the two-step procedure more efficient and flexible for determining the "optimal" factor settings.

When there is no adjustment factors, the objective of the NTB problems becomes very similar to that of the STB and LTB problems. Their primary goal is to move the mean response as close to target (zero for STB and infinity for LTB) as possible while maintaining a reasonably small variability. Although Taguchi recommends using mean squared error (defined on $1/y$ for LTB problems) as the single optimization criterion in these cases, a reasonable alternative is to move the mean as close to target as possible, subject to the constraint that variability is smaller than a certain bound. Vining and Myers (1990) proposed a formal "dual response approach" for doing this. Alternatively, trade-offs between bias and variability can be done informally. This approach helps the experimenter understand the proportions of bias and variance which contributes to the total source of variation and thus provide information for future process improvement.

Note that the strategies described above assume that the loss is quadratic, there is a single response, and the response is static. In practice, these assumptions are sometimes not satisfied.

The quadratic loss function is a reasonable approximation of the true loss function when very little information about actual loss is available. When more information on actual loss is available, methods to approximate the real loss function based on engineering knowledge and customer expectation will significantly improve the efficiency of the robust design method. Leon and Wu (1989) considered a more general class of loss functions when location and dispersion measures can be decomposed. Ng and Tsui (1992) developed a more accurate, complete, and customer-oriented yield measure for product quality improvement. This new measure combines the advantages of a continuous loss function and the yield measure.

Dynamic response is a response whose desired value is not fixed (see Phadke, 1989 for more details). Robust design problems with dynamic responses are very common in practice. So far very few methods have been developed for solving robust design problems with dynamic responses due to the complexity of the problems. Leon et al. (1987) laid out simple models for dynamic response problems and explained how a two-step procedure is possible. Miller and Wu (1991) studied the problem of improving a calibration system with dynamic responses. More research on developing useful strategies for dynamic response problems is highly desired. McCaskey and Tsui (1994) compared alternative methods for analyzing dynamic responses and find that Taguchi's method is not always appropriate for some problems.

6 Alternative Modeling Approach for Robust Design

In terms of statistical modeling, Taguchi's approach is a special case of the following loss model approach: first compute estimates of loss measures (Taguchi proposed the SN ratio as the loss measure), and then fit a model to these loss estimates (see Shoemaker et al. (1991) for more detail).

The loss model approach is appealing because it provides direct estimates of the loss. However, an appropriate statistical model for the loss is often complicated since the loss is a nonlinear and many-to-one transformation of the observed response. A natural alternative modeling approach is to first model the observed response, and then determine the "optimal" factor settings from the fitted model. This approach is called the response model approach and was first proposed in Welch et al. (1990) as a formal procedure. Shoemaker, Tsui, and Wu (1991) further explained the advantages of this approach and illustrated the approach with a physical experiment. Examples of informally modeling responses could be found in Pignatiello and Ramberg (1985) and some early case studies in American Supplier Institute (1984, 1985). Related approaches have also been discussed by Box and Jones (1990), Freeny and Nair (1991), Lucas (1990), Montgomery (1991), and Myers, Khuri and Vining (1991).

As shown in Shoemaker, Tsui, and Wu (1991), Shoemaker and Tsui (1993), and Tsui (1992, 1993, 1994a, 1994b), Taguchi's loss model approach may lead to non-optimal solutions, efficiency loss, and information loss. Below we discuss and illustrate these three problems in detail.

6.1 Unnecessary Bias and Incorrect Design Factor Settings

Assume that the true model of the response (Y) is linear in p control factors $(C_1, C_2, ..., C_p)$ and linear in q noise factors $(N_1, N_2, ..., N_q)$ but each control factor C_i interacts with each noise factor N_j, i.e.,

$$Y = \mu + \boldsymbol{\alpha}^T \mathbf{C} + \sum_{j=1}^{q} (\gamma_j + \boldsymbol{\beta}_j{}^T \mathbf{C}) N_j + \epsilon , \tag{6.1}$$

where $\mathbf{C} = (C_1, ..., C_p)^T$, $\boldsymbol{\alpha} = (\alpha_1, ..., \alpha_p)^T$, $\boldsymbol{\beta}_j = (\beta_{j1}, ..., \beta_{jp})^T$, $N_j' s$ are indepentdently distributed with mean zero and variance $\sigma_{N_j}{}^2$, ϵ distributed with mean zero and variance σ^2, and N_j and ϵ are independent.

Suppose the experiment is a saturated or a resolution III design for the control factors (control array) crossed with a similar design for the noise factors. This implies that the control factor main effects are confounded with the control-by-control interactions and the noise factor main effects are confounded with the noise-by-noise interactions. Since there are no control-by-control and noise-by-noise interactions in model (6.1), if the experimenter models the response (Y) directly, he/she can obtain unbiased estimates of all effects in the response model in (6.1). In contrast, as noted by Box and Jones (1990) and others, the variance of response due to noise and random error under model (6.1) is quadratic in the control factors (C) even though the original response is linear in C , i.e.,

$$\begin{aligned} VAR(Y) &= \sum_{j=1}^{q} (\gamma_j + \boldsymbol{\beta}_j{}^T \mathbf{C})^2 \sigma_{N_j}{}^2 + \sigma^2 \\ &= \sum_{j=1}^{q} \gamma_j{}^2 \sigma_{N_j}{}^2 + 2(\sum_{j=1}^{q} \gamma_j \sigma_{N_j}{}^2 \boldsymbol{\beta}_j)^T \mathbf{C} + \mathbf{C}^T (\sum_{j=1}^{q} \sigma_{N_j}{}^2 \boldsymbol{\beta}_j \boldsymbol{\beta}_j{}^T) \mathbf{C} + \sigma^2 . \end{aligned} \tag{6.2}$$

Since there are control-by-control interaction terms in (6.2), if one models the variance (VAR(Y)) in (6.2) instead of the response (Y), the control factor main effects will be confounded with the control-by-control interactions. Obviously, if these interactions are large, the main effect estimates of the variance model will be seriously biased. As illustrated later, these biases could lead to non-optimal factor combinations in robust design problems.

In general, under model (6.1), if the experimental design allows confounding between main effects and 2-factor interactions in the control array (i.e., resolution III), there will be a bias problem in the loss model approach when the confounded effects are large (since variance is quadratic in C_i's). As shown in equation (6.2), the coefficients of the C_i main effect and $C_i \times C_k$ interaction are equal to $\sum_{j=1}^{q} {\sigma_{N_j}}^2 \gamma_j \beta_{ji}$ and $\sum_{j=1}^{q} {\sigma_{N_j}}^2 \beta_{ji} \beta_{jk}$ respectively. Clearly these coefficients equal zero if the variances of all noise factors (${\sigma_{N_j}}^{2\prime}s$) are zero. Also, since $\gamma_j \beta_{ji}$ and $\beta_{ji}\beta_{jk}$ can be negative, these coefficients can drop to zero due to cancellation of positive and negative terms. In practice, however, it is unlikely that all ${\sigma_{N_j}}^{2\prime}s$ are (close to) zero or the sum drops to zero due to cancellation.

As pointed out in Shoemaker and Tsui (1993), the interaction coefficients would drop to zero if there is separability in model (6.1), (i.e., each noise factor only interacts with at most one control factor). That is, in the coefficient of the interaction, ${\sigma_{N_j}}^2 \beta_{ji}\beta_{jk}$, if either β_{ji} or β_{jk} is zero for each j. On the othe hand, if the noise factor interacts with more than one control factor, the interactions between these control factors in the variance model will be non-zero. For example, if there are six control factors that interact with the same noise factor, this will contribute "6 choose 2" (15) control-by-control interactions to the variance model. When there are multiple noise factors, there is a high chance to have nonzero interaction coefficients. Thus, the effect estimates calculated from the loss model approach under a highly fractionated (resolution III) control array are almost always biased. The next example, based on an experiment reported in Engel (1992), illustrates how the loss model approach results in some seriously biased effect estimates, and thus leads to non-optimal control factor combinations.

An experiment was performed to study the influence of several controllable factors on the mean value and the variation in the percentage of shrinkage of products made by injection molding. This experiment was first reported in Engel (1992) and analyzed by a generalized linear model approach. Steinberg and Bursztyn (1994) re-analyzed the data and pointed out the bias problem of modeling dispersion measures.

Seven control factors A, B, C, D, E, F, and G were tested in a 2^{7-4} saturated fractional factorial design. For each control combination, three noise factors M, N, and O were tested in a 2^{3-1} design. This results into a product array as shown in Table 6.1. The actual names of the control and noise factors are given in Table 6.2.

Following the loss model approach, the variance over the noise runs for each control combination was first calculated (log variance will be considered later). Then the effect estimates of all seven main effects were computed and shown in Table 6.3 together with their alias structure. Note that each main effect is still confounded with three

2-factor interactions even though third or higher order interactions are assumed to be zero. Obviously, the main effect estimates will be biased if the 2-factor interactions are non-zero. Based on a main effect analysis, (i.e. 2-factor interactions are assumed to be zero), main effects C, E, and F were identified to be significant. Factor combination $C = -1, E = 1, F = -1$ with other factors set at arbitrary settings was identified as the "best" control factor setting. Below we will investigate the possible biases of the loss model effect estimates and the correctness of this "best" setting.

Following the response model approach, we first modeled the response as a function of both the control and noise factors. The following fitted model was obtained using the method in Lenth (1989) under the assumption that main effects are more important than 2-factor interactions. Note that among all the effects only A, D, G, CN, and EN are significant. We included effects C, E, and N since their corresponding interactions are significant.

$$\begin{aligned} \hat{y} &= 2.25 + 0.425A + 0.063C - 0.282D + 0.144E - 0.231G \\ &+ 0.138N + 0.45CN - 0.419EN \end{aligned} \tag{6.3}$$

In order to study the biases of the effect estimates in Table 6.3, we follow the approach in Box and Jones (1990) and Myers et. al (1992) to estimate the process variance. By assuming that the noise variables M, N and O in (6.3) are uncorrelated random variables with variances σ_M^2, σ_N^2, and σ_O^2, the variance of $\hat{y}$ over the noise can be estimated by

$$\begin{aligned} \hat{Var}(\hat{y}) &= (0.138 + 0.45C - 0.419E)^2\sigma_N^2 \\ &= (0.019 + 0.124C - 0.116E - 0.377CE + 0.203C^2 + 0.176E^2)\sigma_N^2, \end{aligned} \tag{6.4}$$

which is clearly a quadratic function of the control factors C and E. If model (6.3) is believed to be adequate, equation (6.4) is a good estimate of the process variance. Thus we can find out the potential bias of the effect estimates in Table 6.3. According to equation (6.4), since the main effect of F is zero and interaction CE is nonzero, the estimate on line 6 (0.934) of Table 6.3 should be an estimate of the interaction CE rather than the main effect F. In other words, the main effect estimate of F is seriously biased with the estimate of CE. As shown in Figure 6.1 or equation (6.4), the control factor setting that minimizes the process variance should be $C = -1, E = -1$ or $C = 1, E = 1$

with other control factors set at arbitrary settings. This is quite different from the result of the main effect analysis of the loss model approach described earlier.

Note that, as shown in Shoemaker and Tsui (1993), since noise factor N interacts with more than one control factor (C and E), the individual interaction plots should not be used to identify the "optimal" factor settings. Figure 6.2 shows these individual interaction plots and indicates that different settings of the control factors C and E do not make much difference for reducing the variation caused by N. However, as shown in Figure 6.1, the combination of $C = -1, E = -1$ or $C = 1, E = 1$ gives much smaller variation caused by N than the other two combinations. This illustrates the danger of using individual interaction plots to identify the "optimal" control factor settings. Shoemaker and Tsui (1993) proposed an analysis strategy that rectifies this problem. Steinberg and Bursztyn (1994) have also studied Figure 6.1 and reached the same conclusion. They have also provided a more complete data analysis including model diagnostics.

As for the terms of C^2 and E^2 in (6.4), they caused the bias of the overall mean estimate of modeling the variance. This bias will not affect the result if all control factors are qualitative with only two levels. However, if some control factors are quantitative, the optimal control factor settings will be affected by these biases. Lorenzen's discussion in Nair (1992) addressed this problem in more detail.

One may suspect that the quadratic effect of the variance measure can be linearized by considering the log transformation of the variance. Table 6.4 shows the effect estimates based on log variance of the raw data from Table 6.1. As shown there, the CE interaction effect estimate is still very large and biases the main effect of F. Thus the log transformation did not help eliminate the quadratic effects in this example. As shown in Tsui (1994a), the second order Taylor series approximation indicates that the quadratic terms of the log variance are negligible only when the magnitude of the control-by-noise interaction is much smaller than that of the noise main effect. In the example above, since the noise main effects and the control-by-noise interactions are of the same order of magnitude, the log transformation is ineffective in linearizing the variance model.

Shoemaker and Tsui (1993) pointed out that there are situations when the loss model approach will not give biased effect estimates. These situations happen when the separability condition is satisfied, i.e. each noise factor in model (6.3) interacts with at most one control factor. These situations may occur when there are very few significant control-by-noise interactions. Nevertheless, even though the experimenter may be fortunate enough to avoid the bias problem from using the loss model approach, he may still suffer other problems. In the following subsections we will illustrate other potential problems of the loss model approach. Additional examples of the bias problem can be found in Tsui (1994a) and Steinberg and Bursztyn (1994).

6.2 Information Loss on Individual Noise Effects

From the response model analysis of the experiment in Engel (1992), the only significant noise factor is N and it interacts with two control factors C and E. This leads to a quadratic model in the variance measure. Now we hypothetically assume that the noise factor N only interacts with one control factor, say C, so that the variance model will be linear in C. In addition, we assume that there is another significant control by noise interaction, DO. That is, the underlying model is assumed to be:

$$\begin{aligned} y &= 2.25 + 0.425A - 0.282D - 0.231G + 0.38N + .35O \\ &+ 0.45CN - 0.419DO + N(0, .1^2) \end{aligned} \tag{6.5}$$

Table 6.5 shows the design and the corresponding constructed data under model (6.5). Based on the loss model approach, the main effect estimates of the variance were calculated. Since the underlying model satisfies the separability condition in Shoemaker and Tsui (1993), the loss model effect estimates are unbiased. Figure 6.3 shows the effects of each factor on the chosen loss measure (variance). It was concluded that control factors C and D were the only significant factors which affected the variation of shrinkage caused by noise factors N and O. However, it is not clear how these two control factors reduce the variation caused by individual noise factors.

Following the response model analysis suggested in Shoemaker et al. (1991), we first fitted a regression model for the response:

$$\hat{y} = 2.25 + 0.413A - 0.282D - 0.230G + 0.372N + 0.320O + 0.463CN - 0.407DO$$

Then we studied the control-by-noise interaction plots to see how the control factors dampen the effect of noise. As shown by the plots in Figures 6.4(a) and 6.4(b), changing the level of cavity thickness (C) from "high" to "low" reduced variation caused by moisture content (N) but not ambient temperature (O). On the other hand, as shown in Figures 6.4(c) and 6.4(d), changing the level of holding pressure (D) from "low " to "high" reduced variation caused by ambient temperature (O) but not moisture content (N). This information may help engineers better understand the physical mechanism of the injection molding process.

This example illustrated that the response model approach not only identifies the control factors which reduce the response variability as the loss model approach does, but also reveals control factor settings that dampen the effects of *individual* noise factors. On the other hand, although sometimes the loss model approach may provide unbiased effect estimates, the approach always aggregates the effects of all noise factors together and does not provide any information about the effect of individual noise factors. In general, the loss model approach often provides less information than the response model approach. Additional examples on the information loss of the loss model approach can be found in Shoemaker et al. (1991).

6.3 Efficiency Loss

It is not surprising that the active introduction of noise increases the efficiency of the experiment since the idea is essentially the same as the blocking idea. Similar to the analysis of blocking experiments, the gain of efficiency can be best obtained by fitting a model on both the control and noise factors, i.e., the response model analysis. As shown in Steinberg and Bursztyn (1993), if the robust design experiments are analyzed by the loss model approach, the gain in efficiency will be much smaller than that of the response model approach. Li and Tsui (1994) have compared the efficiencies of the two modeling approaches for a large class of models. Below we will illustrate the comparison using one of their examples.

In this example, we assume the underlying true model of the experiment is as follows:

$$Y = \alpha_0 + \alpha C + \beta_N N + \beta_{CN} CN + \epsilon, \tag{6.6}$$

where $\epsilon \sim N(0, \sigma^2)$, and C and N are the control and noise factors respectively. Suppose the noise factor N follows a random distribution, independent of ϵ, with mean zero and variance σ_N^2 during production. It follows that the process variance caused by the noise factor N is

$$\sigma^2(C) \quad = \quad VAR_{N,\epsilon}(Y) = (\beta_N + C\beta_{CN})^2\sigma_N^2 + \sigma^2.$$

We further assume that there are only two possible values for the control factor C, say 1 and -1. Then the robust design problem is to determine which of the two control factor settings will give a smaller process variance. This problem is equivalent to testing the null hypothesis $H_0 : \sigma^2(1) = \sigma^2(-1)$, where $\sigma^2(1) = (\beta_N + \beta_{CN})^2\sigma_N^2 + \sigma^2$ and $\sigma^2(-1) = (\beta_N - \beta_{CN})^2\sigma_N^2 + \sigma^2$. In terms of the coefficients in (6.6), the null hypothesis is equivalent to $H_0' : \beta_N = 0 \text{ or } \beta_{CN} = 0$.

For the loss model approach, similar to Steinberg and Bursztyn (1993), an F-test based on the sample variances (calculated over a noise design) at $C = 1$ and -1 is used to test the null hypothesis H_0. H_0 is rejected if the ratio of the two sample variances is too small or too large. For the response model approach, a t-test based on the estimates of β_N and β_{CN} is used to test the null hypothesis H_0'. H_0' is rejected if the absolute values of the t-ratios for both β_N and β_{CN} are too large. Li and Tsui (1994) provide the technical detail of the power functions for these two tests. Figure 6.5 shows a plot of the power function at various values of β_N and β_{CN} for the the two tests at significant level 0.05. Clearly the response model approach is uniformly more powerful than the loss model approach with the difference being very significant.

This example is for the case of one control and one noise factor, which may not be realistic. The efficiency comparisons for multiple control factors and multiple noise factors have been studied by Li and Tsui (1994). It was found that the response model approach is always more powerful than the loss model approach for detecting dispersion effects in robust design experiments when a correct model is fitted for the response model. Steinberg and Bursztyn (1993) have also done a similar study by considering a different hypothesis and obtained similar conclusions.

Although the response model approach has become a very useful alternative for modeling and analyzing robust design experiments, the development of the methodology is not yet mature. Important research problems include the incorporation of physical knowledge for simplifying model, the approximation of loss from the fitted response model, diagnostic check of the fitted model (especially for computer experiments), and modeling techniques for experiments with random noise. Shoemaker and Tsui (1992a, 1993) discussed these problems in more detail.

7 Real-World Applications

As described in Section 4, Tsui (1989) developed an algorithm for constructing orthogonal array experiments for mixed setting factors. This algorithm had been implemented in the automatic experiment planner (AEP) module of the AT&T Quality Workbench — ROBUST, a software product to help engineers run robust design experiments. The AEP module requests from users minimum input information and gives as output orthogonal array plans with good statistical properties. This module has been well-received by many AT&T engineers as a very efficient tool for planning experiments. We are currently expanding the class of orthogonal array experiments in the AEP algorithm. The result will be implemented in software eventually. In addition, alternative performance measures developed in Tsui (1990) were also included in the software, which provides users more flexibility for analyzing experiments.

The response model analysis techniques developed in Shoemaker, Tsui, and Wu (1991) and Shoemaker and Tsui (1993) have been applied to re-analyze several case studies in American Suppliers Institute (1985-90). The results have been documented and will be included in the workshops conducted in AT&T and Georgia Tech. In addition, we have worked with AT&T on the application of developed design and analysis techniques to new case studies in IC fabrication process design and circuit designs through computer-aided design systems.

Robust design has been recognized as a very important method for making product performance insensitive to disturbance and thus improving quality of product or manufacturing process design at low cost. We believe that routine use of the robust design method can help push quality activity to the design and development stage and improve product quality efficiently and economically. The implementation of the developed design and analysis methods on robust design have allowed engineers to effectively plan the experiment and efficiently analyze the data by themselves, and thus increased the use of robust design for quality improvement.

8 Summary

This paper reported on some of the research we have conducted over the last two years concerning the development of improved statistical methods for designing and analyzing robust design experiments. In particular, our research addressed the following classes of topics:

- Alternative experimental format for robust design.
- Alternative design criteria and strategies.
- Alternative design techniques and tools.
- Alternative optimization strategies.
- Alternative modeling approach.

We have enjoyed a great deal of success with this line of research. We believe that there is a great deal of interesting research left to be done in these areas.

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Table 6.1 : Design and data of the injection molding experiment

Run	Control Array							Noise Array			
							r	-1	-1	1	1
							s	-1	1	-1	1
							t	-1	1	1	-1
	A	B	C	D	E	F	G	Data			
1	-1	-1	-1	-1	-1	-1	-1	2.2	2.1	2.3	2.3
2	-1	-1	-1	1	1	1	1	0.3	2.5	2.7	0.3
3	-1	1	1	-1	-1	1	1	0.5	3.1	0.4	2.8
4	-1	1	1	1	1	-1	-1	2.0	1.9	1.8	2.0
5	1	-1	1	-1	1	-1	1	3.0	3.1	3.0	3.0
6	1	-1	1	1	-1	1	-1	2.1	4.2	1.0	3.1
7	1	1	-1	-1	1	1	-1	4.0	1.9	4.6	2.2
8	1	1	-1	1	-1	-1	1	2.0	1.9	1.9	1.8

Table 6.2 : Factors in the injection molding experiment

Control Factors	Noise Factors
A: cycle time	M: percentage regrind
B: mold temperature	N: moisture content
C: cavity thickness	O: ambient temperature
D: holding pressure	
E: injection speed	
F: holding time	
G: gate size	

Table 6.3 : Effect estimates and their aliases for variance

Effect Estimates	Aliased Effects			
	m.e.	2-f.i.		
-.030	A	BC	DE	FG
.028	B	AC	DF	EG
.055	C	AB	DG	EF
-.027	D	AE	BF	CG
-.056	E	AD	BG	CF
.934	F	AG	BD	CE
.028	G	AF	BE	CD

Table 6.4 : Effect estimates and their aliases for log variance

Effect Estimates	Aliased Effects			
	m.e.	2-f.i.		
-.217	A	BC	DE	FG
.136	B	AC	DF	EG
-.094	C	AB	DG	EF
.109	D	AE	BF	CG
-.152	E	AD	BG	CF
2.862	F	AG	BD	CE
-.187	G	AF	BE	CD

Table 6.5 : Design and data of the constructed example

Run	Control Array							Noise Array			
							r	-1	-1	1	1
							s	-1	1	-1	1
							t	-1	1	1	-1
	A	B	C	D	E	F	G	Data			
1	-1	-1	-1	-1	-1	-1	-1	1.58	2.74	3.24	1.65
2	-1	-1	-1	1	1	1	1	1.41	1.14	1.30	1.30
3	-1	1	1	-1	-1	1	1	0.43	3.39	1.86	1.90
4	-1	1	1	1	1	-1	-1	1.04	2.60	0.90	2.75
5	1	-1	1	-1	1	-1	1	1.19	4.35	2.51	2.81
6	1	-1	1	1	-1	1	-1	1.89	3.43	1.52	3.48
7	1	1	-1	-1	1	1	-1	2.46	3.93	3.97	2.34
8	1	1	-1	1	-1	-1	1	2.39	1.89	2.19	2.10